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ECOLOGY AND THE SCIENCE OF PSYCHOLOGY

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In the hierarchy of life sciences, psychology occupies a place between the biological disciplines, which focus on the structure, development, and functioning of organisms and the social science disciplines in which the significant unit of observation is a population group. Psychology, which is both biological and social, takes the molar behavior of the individual in his physical and social setting as its observational unit.

A major concern of the biological and social sciences has long been the nature of the interaction of organisms and populations with the embedding environment, which supports, influences, and determines limits of structure and function for the life that exists within its domain. The generic term representing scientific study of organism-environment interaction is Ecology.

Traditionally, Ecology has been recognized as a branch of biology, while the term Human Ecology has been used to

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designate the investigations, principally by sociologists and geographers, of the distributions of human population groups in relation to material resources, health, social, economic, and cultural patterns. These disciplines have produced significant bodies of knowledge and theory and they have developed distinctive journals, literature, and learned societies.

No such formal development has yet occurred in psychology, although an ecologic emphasis has become pronounced in recent years and may well be gathering enough momentum to crystallize as a major trend in the next decade. If this should happen, however, it will undoubtedly be a protest movement against strongly intrenched traditions which may indeed have themselves evolved as ecologic phenomena in the development of the science. This point will be developed later. The main thrust of this paper, however, is to argue the case for an ecologic emphasis in psychology.

The discussion will review some significant, converging developments contributing to the ecologic trend in psychology and also some issues and difficulties that must be faced and eventually resolved. This presentation is focused principally on implications of the ecologic emphasis on content and method

in psychology. If it should appear to be controversial, this is intended. The issues with which we are concerned have too long been ignored.

ECOLOGY AND THE CONTENT OF PSYCHOLOGY

Although psychologists are traditionally self-critical, and have been viewing with alarm the directions of our corporate development for many years, my sensitive ear has recorded increased turbulence in recent months. The foment does not appear in any sense uni-dimensional. The most pronounced theme expresses serious concern with the preoccupation of psychologists with what various critics have characterized as hollow, unproductive, artificial, and even pointless activities in many animal and so-called experimental laboratories and urges attention to realistic human problems; see, for example, the papers by Bakan, N. Sanford, and Berg in the March 1965 issue of the *American Psychologist*. Others focus on *theoretical orientations*, such as the Type I vs. Type II theory controversy (Winthrop, 1965) and the related opposition of behavioristic and cognitive theories, and on *methodology*, particularly the growing opposition of multivariate advocates to the univariate, experimental tradition (Sells, 1963; Barker, 1965). These theoretical

and methodological issues also have implications for choice of content.

As in most heated controversies, an impartial judge can see reason on both sides of each confrontation. Our purpose is not to choose sides, but rather to arrive at a rational position for the science of psychology with regard to its objectives, choice of problems, and its methods. This issue is not related in any way to individual interests in choice of areas of personal involvement.

Phylogenetic Perspective

Our point of departure is the citation of a number of generalizations that appear to be widely accepted. The first is the objective fact that the behavior of organisms is rooted in biological development. As a result behavior mechanisms have developed and must be understood in phylogenetic perspective. Even the psychologist who views practical human problems as predominant must acknowledge that a thorough science of psychology embraces the entire range of living species. Indeed, the interdependence of structure and function makes it imperative for the behavioral scientist to understand both the long- and short-term development of the biological structures and

systems that both determine and are determined by the molar behavior of the organism (Murphy, 1947, 1958).

One distinguished psychologist (Hebb, 1958) has stated his position on this issue as follows:

> "The most pressing problems of behavior are those of mental illness and social conflict... The 'pure-science' approach, the development of theoretical understanding, complements the practical approach by providing the only guarantee of better methods in the future. Mental illness involves perception, memory, emotion, thinking; so does the attitude of hostility to other peoples. But we do not fully understand perception, memory, and so on; thus, anything that tells us more about these processes, whether it is the study of the eye-blink in man or a study of the mating habits of the rat, is a potential addition to our understanding of mental illness or the causes of social conflict."

These words of Hebb may sound persuasive. Yet, it is not necessary to assume that the illumination of pressing human problems constitutes the *raison d'etre* of our science. The phylogenetic perspective is intrinsic to its structure. *Homo sapiens*, as well as every other species, is an emergent member of a complex, dynamic system of living organisms, which reflects systematic continuities, even in the face of observed discontinuities, and in which historical position and relationship provide significant insights concerning structure and function. In this system studies of eye-blink phenomena and mating habits of any

species have only the most limited meaning when pursued in isolation, as ends in themselves, but make their greatest potential contributions when related to an overall periodic table of phylogenetically organized and oreinted data.

<u>Adaptation</u>

In order to organize the science of behavior phylogenetically, our psychological approaches must take account of the most pervasive characteristic of biological systems, the principle of <u>adaptation</u>. Essentially this involves the self-regulating tendency of living organisms to maintain themselves by various means of accommodating or adjusting to changes in the environment. At different levels the principle of adaptation can be observed in the mechanisms involved in species change, in growth, tissue regeneration, regulation of biological functions, and in molar behavior processes, such as learning and motivation. In an earlier paper (Sells, 1963), the writer has dealt at some length with these phenomena as interactions, which are systematic and adaptive rather than random encounters between inner and outer forces.

Objections have been raised to the concept of adaptation on the grounds of its "purposive" overtones. However, I am inclined to view these as primarily semantic. It is recognized

that the concept has been frequently misinterpreted. But at the operational level scientific descriptions of adaptive processes, as for example, Dobzhansky's (1962) account of natural selection, Davis' (1961) description of cold acclimatization, Pribram's (1960) discussion of variable, tunable homeostats, and the more recent literature on the Law of Effect in learning (Kimble, 1961), are straightforward, deterministic accounts of observed events.

Natural selection occurs when two or more classes of individuals, genotypically distinct in some characteristic, transmit their genes to succeeding generations at different rates and one is better able than the others to survive under prevailing environmental conditions. Purpose is unnecessary to the linkage of the events described. The record of survival and extinction of classes differentially adapted to environmental change, though incomplete and not without problems, supports this conclusion, and the evidence is considered tenable that cumulative change of a magnitude recognized as evolution of "higher" types occurs as progressive adaptive change. Thus, the evolutionary progression to Hominidae, Hominae, Homo, and Homo Sapiens discloses the appearance of new patterns of behavior reflecting the sequential, cumulative effects of complex new organism-environment interactions continually enabled by the development of emergent

structures. The occurrence of these causally-linked events, in which complex consequents follow antecedent change, appears to be as lawful and untainted with hormic implications as any of the phenomena of gravity or thermodynamics.

The Feedback Model

Adaptive function of an organism also implies the existence of feedback mechanisms. The posture of the organism at any moment is in effect the expression of an intrinsic (and not necessarily consciously experienced) hypothesis concerning the nature of the environment. Every response is similarly an interrogation of the environment and the resulting feedback provides information (also not necessarily conscious) that enables adaptive response. Without attempting to review the extensive literature in several fields, our present position is that the existence of biologic and neurophysiological feedback systems is a necessary assumption about adaptive organisms.

The adoption of computer terminology by psychologists has made it possible to discuss anticipation and information inherent in feedback mechanisms without resorting to anthropomorphic expressions. This is apparent in the substitution by Cofer and Appley (1964) of the equilibration model for the need-reduction model in their discussion of feedback systems relevant

to motivation and in their proposed sensitization (SIM) and anticipation (AIM) mechanisms as proposed models for motivation theory. Other noteworthy developments incorporating or implying feedback concepts include Woodworth's comprehensive law of homogeneity (1938), Hebb's concept of response equivalence (1949), the concept of reinforcement in learning, Piaget's concepts of accommodation and assimilation (Hunt, 1961), Helson's adaptation level theory (1963), the TOTE unit of Miller, Galanter, and Pribram (1960), the Sherif's (1956) principles of stimulus structure and psychological selectivity in perception, and the information-processing model of motivation presented by Hunt (1963).

The specific nature of the biochemical and bioelectric feedback mechanisms involved in adaptive biologic function and behavior is not yet clear, but is not essential to the present discussion. However, our argument does depend on the assumption that such mechanisms are essential components of adaptive organisms and both biologic functions and molar behavior are controlled by information-processing systems in which anticipation, expectation, posture, or set is a fundamental characteristic, and adjustment, based on information-feedback, is another.

The Ecologic Niche as Context

These characteristics of adaptive behavior emphasize the importance of the environment with which organisms interact in biological and behavioral development and function. This interdependence was perhaps most critically appreciated by Ashby (1962) who faced the problem of specification of environmental inputs in his computer model of a brain. Here the rigorous requirements for detailed information demonstrate the inadequacy of the often-cited global expression of interaction, $R = f\ (I \times E)$, in which R (behavior) is a function (f) of the interaction of organism (I) and environment (E). Not only must the nature of the function term be explicated, but the variables comprising the I and E sets must be specified and measured before such an equation can be realistically useful.

The arguments advanced thus far are the basis for four postulates concerning the strategic role of the ecologic niche in the structure of behavior. First, it is recognized that every species and class of living organisms has evolved by adaptation to a particular set of environmental conditions with reference to which its morphologic structures, physiologic systems, and behavior response repertoire are optimally suited. This environmental pattern is defined as the ecologic niche and represents

the adaptive match between circumstances and species schema (Hunt, 1961).

Second, these very structures, physiological systems, and repertoires of responses, defined by the ecologic niche, are the characteristics that are strategically relevant to the periodic table of phylogenetically oriented data referred to above. Focusing on behavior, it may be said that the response repertoire represents the natural way in which a species "makes its living" in its natural environment.

Third, the differences between species in historical position, critical environment, and related response repertoires all limit the types of cross-species comparisons that may be meaningful and suggest types of comparative studies that may profitably illuminate behavior in phylogenetic perspective.

And finally, the understanding of behavior requires systematic study of the characteristics of the environmental pattern defining the ecologic niche of each species and the adaptations required by that environment as well as of response processes.

These postulates are the basis for two contextual prescriptions concerning the content of psychology: (1) There is a need for a master plan, corresponding to the periodic table mentioned above, to place psychological research in phylogenetic

perspective. Such a schematic analysis could serve as a systematic guide to the definition of empirical information required for a comprehensive science of psychology. Considering the aspirations of psychology as a systematic science, it is sobering to consider the limited range of our knowledge of species outside of rats, chicks, monkeys, college students, military basic trainees, babies, and hospital patients. (2) For every species and class this schematic table should contain a detailed description and quantitative analysis of both the features of the environment defining the ecologic niche and the matching behavior response repertoire.

The conspicuous lack of such normative data is strongly voiced in the following remarks by Roger Barker (1965) in a recent address to the American Psychological Association:

> "This state of affairs is most surprising in view of the situation in the old, prestigeful sciences which psychology so admires and emulates in other respects. In these sciences, the quest for the phenomena of science as they occur unaltered by the techniques of search and discovery is a central, continuing task; and the development of techniques for identifying entities and signaling processes without altering them (within organisms, within cells, within physical systems, and within machines) is among the sciences' most valued achievements. Handbooks and encyclopedias attest to the success of these efforts. I read, for example, that potassium (K) ranks seventh in order of abundance of elements,

and constitutes about 2.59% of the igneous rocks of the earth's crust; that its components are widely distributed in the primary rocks, the oceans, the soil, plants, and animals; and that soluble potassium salts are present in all fertile soils. (Encyclopedia Britannica, 1962). The fact that there is no equivalent information in the literature of scientific psychology (about playing, about laughing, about talking, about being valued and devalued, about conflict, about failure) confronts psychologists with a monumental incompleted task."

The programmatic work of Kinsey and his associates on human sexual behavior (1948, 1953) and of Barker and the staff at the Midwest Psychological Field Station, University of Kansas, on recording units in the "stream of behavior" of children in several locales has demonstrated the importance of ecologic norms for understanding particular problems (1963). For example, Clifford Fawl (1963), a student of Barker's found that when referring to observational records of childrens' behavior in their natural surrounds, as contrasted with the psychological laboratory, frustration occurred rarely, and when it did occur, it did not have the behavioral consequences observed in the laboratory (Barker, 1965). Barker commented on this as follows:

> "It appears that the earlier experiments (by Lewin, Dembo, and Barker) simulated behavior very well as we defined and prescribed it for subjects (in accordance with our theories); but the experiments did not simulate frustration as life prescribes it for children."

We may wonder about the relation of other experimental treatments of segments of behavior to the occurrence of the designated behaviors in real-life situations.

One other example, from a paper by Gump and Kounin (1960), the former a student of Barker's, illustrates this point further. Gump and Sutton-Smith (1955) investigated the reactions of poorly skilled players when they were put in more or less difficult game positions or roles. For example, in games of "tag," the _it_ position is more demanding under some rules than others.

As they played the game experimentally, an _it_ in the center of a rectangular playing field attempts to tag opponents who run to and from _safe_ areas at each end of the rectangle. One variant of the game gives _high power_ to the _it_ position by permitting the child in that position to "call the turn" when runners may attempt to cross from one safe position to another. Another variant gives _low power_ to the _it_ by permitting players to run whenever they choose. In one phase of the experiment, slow runners were assigned to high-power _it_ positions, and in another, to low-power _it_ positions.

The hypotheses, that poorly skilled boys would be more successful in high-power than low-power _it_ positions, and that

scape-goating of these inept boys would be less frequent in the high- than low-power positions was unequivocally confirmed. However, the authors also observed boys in natural, rather than experimental situations, in gyms, playgrounds, and camps, and obtained the following impressions:

> "(a) poorly skilled boys do not often get involved in games they cannot manage; (b) if they do get involved, they often manage to avoid difficult roles by not trying to win such a position or by quitting if they cannot avoid it; and (c) if they occupy the role and are having trouble, the game often gets so boring to opponents that these opponents let themselves be caught in order to put the game back on a more zestful level."

The experimental game, which was watched by adults, was artificial in that poor competitors were inappropriately placed in skilled positions and in these circumstances no opponent ever let himself be caught. It actually created a highly unusual and, to the children, desperate circumstance, in which interfering adults were intervening agents. Gump and Kounin concluded that "The experiment probably does suggest hypotheses as to why certain games and roles are avoided by inept children, and it shows the extent to which children can scapegoat when conditions are artificially favorable."

For the present discussion these studies symbolize a host of experimental analyses of behaviors that occur only in

the artificial and unrealistic experimental situations contrived to fit the hypotheses of experimenters who are ignorant of or indifferent to the conditions of occurrence of those behaviors in their natural settings.

Although the position advocated here favors multivariate field observation and experiment over univariate laboratory experiment as the methods of choice in psychological research, the value of the laboratory as an adjunct to the field research station, to isolate, test, verify, and replicate particular aspects of phenomena observed in natural settings, should not be overlooked. What is objected to is the view that has become orthodox policy in many university circles, that the laboratory is supreme, that investigation without experimental manipulation of treatments and testing of hypotheses is unworthy of the term research, and that laboratory environments are preferable to natural settings. Unless the laboratory is a valid simulation of the conditions of the natural setting, in our opinion, it may fail in its primary purpose of contributing to the understanding of behavior, even though it may well serve the economic goal of producing more sterile Ph.D.'s.

In closing this section on the ecologic niche as the context for scientific study of behavior, it is fitting to recall

the 1961 report of Keller and Marian Breland which not only recognized the primary relevance of the ecologically matched behavior response repertoire, but presented evidence of the prepotency of such responses over others to which animals were conditioned in the laboratory.

The Brelands, who built a highly successful business on the operant conditioning of animals for commercial exhibits, encountered a number of "disconcerting failures" in which conditioned animals, after thousands of reinforcements of specific (ecologically artificial) learned responses, gradually drifted into behaviors entirely different from those which were conditioned.

The direction of this drift was toward what the authors called "instinctive behaviors having to do with the natural food-getting behaviors of the particular species." For example, "dancing chickens" drifted toward persistent scratching behavior, which is a prominent feature of the natural response repertoire of gallinaceous birds in their native habitat; chickens that were conditioned to peck a souvenir toy onto a chute drifted toward hammering them apart, thereby reverting to their natural habit of breaking open seed pods, killing insects, and the like; and the pig who was trained to pick up a coin and carry it to the bank, gradually began to drop it and "root it along the way," thus

demonstrating the prepotency of a strongly established food-getting component of the natural repertoire of his species.

The Brelands were as explicit in disclaiming any intention of disparaging conditioning techniques *per se* as we are of disparaging the experimental method, *per se*. It is really providential that the early success of their enterprise resulted in the continuation of many of their "experiments" for a sufficiently long period (far beyond the duration of most laboratory experiments) that the so-called "instinctive drift" could occur. Their scientific insights may be regarded as a handsome repayment to academic science whose "impeccable empiricism" they have extensively exploited.

ECOLOGY AND METHODS OF PSYCHOLOGY

Acceptance of the ecologic principles discussed above has a number of profound implications for the scientific methods of psychology. In the first place, this position requires methods that are adapted to the circumstances of the real world in which behavior occurs naturally rather than to the convenience of the scientist, and with them, perhaps, a new generation of field-oriented, pioneers in psychology, to go forth into the world which we, their teachers, have foresaken for the often delusive

comforts of the automated ivory tower. However, this world of reality is complicated, often uncontrollable, and not always tolerant of prying investigators. It operates according to rules of privacy and inviolacy and at a pace that often seems to defy investigative resourcefulness. These are problems that have beset many an unwary investigator and they have long been appreciated. They enable us to understand, if not approve, the advantages of the uncomplicated, traditional, laboratory milieu. Unfortunately the more removed from reality it becomes, the more illusory the science will be.

What is the resolution of the dilemma? First, the need is indicated for vastly greater communication and cooperation between psychology and both the bio- and social-sciences, for the methodology most appropriate to many of the fundamental psychological problems is necessarily interdisciplinary. Most present undergraduate and graduate training in psychology is inadequate in this respect.

Source data on major parameters of both the physical and social environmental and behavioral effects have been compiled and published by oceanographers, climatologists, limnologists, physicists, geologists, geographers, anthropologists, sociologists, and even psychologists, as well as many other disciplines.

The following examples are mentioned merely to illustrate the wide range of excellent material available: Tromp's (1963) comprehensive work on medical biometeorology, dealing with weather, climate, and the living organism; environmental and operational data on the Polar Basin and the Arctic Region, in Sater's (1963) report for the Arctic Institute of North America; the effects of unusual environments encountered by men in military and space operations, by Burns, Chambers, and Hendler, psychologists (1963); extensive files on the detailed social, economic, political, legal, judicial, religious, health, familial, educational, scientific, military, and behavioral structure, folkways, and norms of the peoples of the world, coordinated by Murdock, Ford, et al. (1937-), Human Relations Area Files, Inc., Yale University; and definitive studies of human social structures, social systems, culture, and social change, in the two volume work edited by Parsons, Shils, Naegele, and Pitts (1961).

Zoologists and other biologists have published extensive material on the behavior of animals in their native habitats. Recent books by Portman (1961) and Etkin (1964) on social behavior and organization of vertebrates illustrate scientific contributions by these colleagues, of value to psychologists.

Last year (Altman and Dittmer, 1964) the Federation of American Societies for Experimental Biology, under Air Force support, published a Biology Data Book, for which 470 botanists, zoologists and medical scientists contributed and reviewed data, which presents authoritative information on the parameters of genetics, reproduction, growth and development, morphology, nutrition and digestion, metabolism, respiration and circulation, blood, biological regulators, environment and survival and other factors for many species of plants, vertebrates, invertebrates and man. While much of the detailed data is of primary interest to biologists, this publication contains much material of psychological interest, such as effects of temperature, shade, and light on growth and survival of many species, effects of exposure to ionizing radiation, estimates of the number of species of plants and animals and taxonomic classifications of living animal species.

The purpose of citing this vast and wide-ranging array of literature is to document the point that the task of compiling a schematic periodic table focused on the environmental niches and behavior repertoires of animal species will receive a large assist from the older sciences that have long ago appreciated its importance. These disciplines have faced and in many cases overcome the difficulties of inaccessibility of observable

phenomena, danger, discomfort, and privacy. Kinsey and his coworkers used a simple, but effective interview technique which paved the way for others to penetrate the seeming iron curtain of mystery and repression surrounding human sexual behavior. The behavior protocols of Barker and his colleagues demonstrated that observational techniques need not be confined to the baby's crib or the conference table. Both of these pioneers produced data of acknowledged richness and importance to the structuring of scientific inquiry in psychology.

In the same vein is a doctoral study at the University of Houston by Zinner (1963), in which observations were recorded of 56 specific behaviors (such as using left, right, or both hands, eating, hands in pockets, drinking, holding an object, non-personal rubbing of shoes, brass or windows, writing, urinating, leaning against something, smoking, talking to one person, saying Grace, smiling, and the like) on 90 Airmen, with the help of 98 specially trained Airman observers. These observations were made over 25 days, without interference by observers, in 30 different real-life situations in Basic Training (such as Wake up, from lights-on until the subject left the personal area, Personal area involving bunk and footlocker, Latrine, Marching to Messhall for breakfast, Table at breakfast, and so on from

4:45, in the morning until 5:00 in the afternoon. Zinner used much ingenuity and care in collecting and analyzing these data and received massive support from the Air Force, which provided subjects, observers, and clerical and computer services for tabulation and multivariate analysis of the data.

In general agreement with a less ambitious observational study of one nine year old boy in two different situations of one day's duration each, by Gump, Schoggen and Redl (1963), Zinner observed a number of dependancies between situations and specific behaviors, for example situations that either facilitated or inhibited smoking or talking in varying degree. Factors extracted from his data were stable over subjects and occasions.

These examples support the belief that there are no insuperable obstacles to effective field observation and no lack of ingenuity among investigators convinced of the importance of the approach. At present, the culture of our psychological establishment is complacent in its scientific posture. If this posture were changed in the directions advocated here, it is easy to visualize the press of aspiring students to get on the band wagon and the willingness of funding agencies to furnish adequate support.

Observation of behavior in its natural setting, without interference or manipulation by the investigator would not only free psychology from the almost insurmountable and ungeneralizable limitations of experimental results generated in situations in which the effects of complicating, but ecologically relevant variables are excluded by experimental control, but also would reduce the equally inescapable difficulty of iatrogenic influences on results, that is, the built-in effects of the experimenter's hypotheses expressed in his particular designs and procedures (Kintz et al., 1965). Even simple invertebrates such as planaria have demonstrated such experimenter effects, according to a report by Rosenthal and Halas (1962).

On the other hand, this enthusiasm must be tempered by concern with the real problems of effective field-oriented methods. There is no question that they will raise the cost of psychological research to an awesome degree, for they involve multivariate observation and analysis on a time scale far in excess of present practices and over numbers of subjects, situations, and replications that imply massive organizations for data collection, reduction, and analysis. But this is not all. Technical difficulties obstruct the way, even if the cost and logistics problems can be solved. The most important technical issues are: (1) The

problem of recording observations appropriately and completely and of reducing observational protocals to reliable units for quantitative analysis, and (2) the problem of encoding the environment.

The first problem may look more difficult than it actually is. Zinner's observations were highly reliable. However, the behaviors he observed were selected to be objectively discrete and overt and his observers were carefully trained. Dickman (1963), a student of Barker's, on the other hand, found that naive beginning psychology students agreed poorly on the analysis into units of continuous samples of behavior presented in a motion picture. Still, improvement occurred with practice.

Dickman also pointed out that "the 'stream of behavior' attains orderliness in the eyes of other humans to the extent that goals and motives are imputed to the behavior. Independent observers of such a behavior continuum demonstrated significant agreement on general patterning and specifically on the points at which units began or ended. They agreed very poorly on identical incidence of units, yet they were able to agree on the general meaning of the sequence." He attributed this paradoxical result to differences of the inclusiveness of goal or behavior perspectives of the judges.

To some extent this problem is reminiscent of the experience of McClelland and his associates (Atkinson, 1958) in their efforts to obtain reliable scoring methods for TAT protocols. I am inclined to agree with Barker (1963) that increased familiarity with observational methods will lead to improvement of these methods.

The problem of encoding the environment is the big hurdle. After five years of fairly continuous work on this problem (Sells, 1963a, 1963b), I am more hopeful than previously. For one thing, it looks more viable in the frame of reference of field study than from the perspective of the laboratory. I believe that the isolation of laboratory performance from the context of a stream of behavior, which Barker has emphasized, prompts one to think of the environment in discrete, molecular terms, while the real life setting suggests the patterned regularity and coherent structure of the familiar physical and social environment. The physical world consists not of discrete bits of light, heat, moisture, and the like, but of patterned events, with system qualities, in which day-night cycles, months, and seasons have associated with them characteristic temperatures, light, climatic variations, activity patterns, modes of dress, and the like. Similarly, the social world consists of patterned events with system qualities embracing multiple discrete stimuli which typically (except in

unusual, extreme conditions) influence behavior, not in isolation, but by virtue of what Sherif (1956) has called their membership character derived from the qualities of the respective systems. In this frame of reference a given unit may belong to more than one system and function differently in each.

I am not ready to renounce the proposed taxonomy of environmental variables outlined in my 1963 book (Sells, 1963) because work in our laboratory (Findikyan and Sells, 1964, 1965) and elsewhere (Friedlander, 1965; Pace, 1958, 1960; Stern, 1956; Thistlewaite, 1959a, 1959b; Astin, 1961, 1962, 1963) has demonstrated the value of multivariate analysis of variables used to measure the more obscure and subtle aspects, at least, of the social environment. Such studies are particularly important, for example, in identifying characteristics of organizations, of social climates, of work situations, and other subsystems of the social environment that may be useful for measurement purposes.

Dependencies between characteristics of situations and behaviors encourage us to extend the exploration of environmental determiners of behavior. Until we can assign to environmental variables the proportions of variance in behavior for which they account, our understanding of behavior will be incomplete.

When this is accomplished, we shall have achieved the goals of this ecologic emphasis and will have returned the science of psychology to its true course.

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